

gentlemen cannot be charged with committing themselves to an hypothesis "the most distant from the phenomena it attempts to explain."

Now if it can be shown that the germs of disease are subject to the same laws as other living things and exhibit similar phenomena, and further, that without the inference that they are endowed with vital properties, it is impossible to unravel the most striking character which they present to us for consideration, viz., the fact that they reproduce their kind, then I think there is more reason for following up, in all its intricacies, the germ theory, than to start with an assumed catalysis, molecular motion, and a glandular matrix, as suggested by Dr. Richardson.

Starting, then, from the indisputable fact that the *materias morbi* of every communicable disease reproduces its kind, I have considered this a primary law, and have tabulated other laws which are associated with living beings by which it will, I think, be found that there is a parallelism of a kind to attract and rivet attention, especially, too, when many otherwise inexplicable circumstances bend to this hypothesis.

Primary Law of Reproduction, by which all living things reproduce their kind.

SECONDARY LAWS.

<i>Objective Laws.</i>	<i>Subjective Laws.</i>
1. The diffusion or dispersion of germs.	1. Seasons of activity.
2. Their static existence.	2. Climatic influence.
3. Limited duration of active existence.	3. Relation to latitude.
4. Period of development, maturity, and decay.	4. Subjection to physical forces.
5. Intermittent reproduction.	5. Influence of locality.

Without amplifying this subject, which would carry me far beyond the limits of an ordinary communication, I will only add that though the above tabulation is very imperfect, there is quite sufficient for any one who will follow out the ideas conveyed by it to trace the intimate relation that exists between living beings and the germs of disease. I would refer finally to the fact that many diseases in men and animals have yielded up living germs as their cause, chiefly, I may add, skin diseases it is true; but *aphtha*,¹ closely associated with diphtheria, is, I think, acknowledged by all unprejudiced persons to have its origin in an unmistakable and demonstrable germ.

JOHN GROVE

The Zoological Relations of Madagascar and Africa

WITHOUT entering into the details of this very difficult question I wish to be allowed to state some of the general reasons which have led me to a different conclusion from Dr. Hartlaub,² and also to point out where he has not quoted my opinions with perfect accuracy. Instead of saying that "the fauna of Madagascar is manifestly of African origin," my actual statement is as follows:—"We have the extraordinary fauna of Madagascar to account for, with its evident main derivation from Africa, yet wanting all the larger and higher African forms; its resemblances to Malaya and to South America; and its wonderful assemblage of altogether peculiar types" ("Geog. Dist. of Animals," vol. i. p. 286). My reasons for believing in the "main derivation" of the fauna from Africa can only be understood by considering the theory, now generally admitted, of the origin of the fauna of Africa itself. All the higher mammalia are believed to have entered it from the northern continent during the middle or latter part of the tertiary period, and the occurrence of *Psittacus* and of forms supposed to be allied to plantain-eaters and to *Leptosomus* in the miocene of France, render it probable that many of the peculiar groups of African birds had their origin in the old Palaearctic region. Now Madagascar presents many cases of special affinity with South Africa, especially in insects, land-shells, and plants; and if we suppose it to have formed part of a South African land before the irruption of the higher mammals and birds from the north, we shall I think account for many of its peculiarities. Such facts as its possessing *Potamocharus* and the recently extinct *Hippopotamus*, while it has thirteen or fourteen peculiarly African genera of birds against four or five that are peculiarly Oriental; of its having many African genera of lizards and tortoises; of its butterflies being decidedly African; of its numerous African genera of Carabidæ, Lucanidæ, and Lamiidæ; while the specially Oriental affinities of its mammals, reptiles,

and insects are hardly if at all more pronounced than the South American affinities of the same groups,—all seem to me to warrant the general conclusion that the "main derivation" of the Madagascar fauna is from Africa.

Dr. Hartlaub speaks of my "attempted parallel between Madagascar and Africa, and the Antilles and South America" in such a way that his readers must think I had dwelt upon this parallel in some detail as being special and peculiar. The fact is, however, that I have always referred to it in a very general way. At p. 75 vol. i. I say: "The peculiarities it (the Malagasy sub-region) exhibits, beings of exactly the same kind as those presented by the Antilles, by New Zealand, and even by Celebes and Ceylon, but in a much greater degree." And again, at p. 272, vol. i., I speak of it as "bearing a similar relation to Africa as the Antilles to Tropical America, or New Zealand to Australia, but possessing a much richer fauna than either of these, and in some respects a more remarkable one even than New Zealand." This general comparison with the two other great insular sub-regions is, I think, justifiable, notwithstanding great differences of detail. There is in all a rich and highly peculiar fauna, a great poverty of mammalia, and a total absence of many large families of birds characterising the adjacent continent, together with special points of resemblance to distant continents or to remote geological periods.

It seems to me that such a problem as this cannot well be solved by means of a group which, like birds, do not require an actual land-connection in order to reach a given country; and, if all land animals are taken into account, the evidence does not appear to warrant the supposition of a recent land-connection of Madagascar with India or Malaya. At a very remote epoch such a connection may have taken place, but if we are to give any weight to the general facts of distribution as opposed to those presented by birds only, the union of Madagascar with South Africa is more recent and has had more influence on the character of the Malagasy fauna. The numerous and very remarkable points of affinity between Madagascar and South America in almost every group except birds, are not alluded to by Dr. Hartlaub, yet they would equally well support the notion of a former union of those two countries independently of Africa. It seems, however, more consonant with our general knowledge of distribution to consider these as cases of survival of ancient and once wide-spread types in suitable areas; and this is a principle that must never be lost sight of in attempting to solve the problems presented by such anomalous countries as Madagascar.

ALFRED R. WALLACE

Selective Discrimination in Insects

YOUR correspondent S.B., in his letter NATURE of yesterday's date, must be referring to some short abstract only of my lecture on flowers and insects. I quite agree with him that odour is very important in attracting insects, and dwelt upon it in my lecture, as well as in my little book on "Flowers and Insects." A striking illustration is afforded by night flowers, which often become peculiarly odoriferous towards evening, as has been already pointed out by various observers.

S.B. attributes, I think, too little importance to the colouring of flowers, but his letter shows him to be a careful observer, and I hope he will continue to devote his attention to the subject.

He would find H. Müller's "Blumen und Insekten" a mine of most interesting and accurate observation.

London, October 19

JOHN LURBOCK

Protective Colouring in Birds

WITH reference to the statement in my "Naturalist in Nicaragua," p. 196, that the macaw "fears no foe," &c., the well-known geologist, Prof. Gabb, sends me the following information:—"I willingly comply with your request to repeat the statement about the *Kukong pung* or macaw hawk of Costa Rica. Not having your book by me now I cannot refer to page nor quote your statement exactly. But as I recall it, you speak of the great red and blue macaw as being so well defended as to need no protective colouring, and that no hawk dares attack it. In this you are mistaken. Not only have I seen on several occasions heaps of the unmistakable feathers of the bird in the woods, left in the manner that all woodsmen recognise as hawk's work, but I have the statements of various Indians, not in collusion, confirming each other, and finally I have had the bird pointed out to me (I am not sure but that it may occur in the collection I sent to the Smithsonian). It is a fair-sized hawk of dark

¹ See *Medical Times*, 1851, vol. ii. p. 95.

² NATURE, vol. xvi. p. 498, and the *Ibis* for July, 1847, p. 334.

colour. It always attacks its prey on the wing, swooping down and disabling it when least able to use its effective weapon. It is well known to the Indians, and its specific name among them indicates its habits—*Kukong* (macaw) *pung* (hawk)—in the same manner as the eagle is called *sar pung* or monkey hawk."

There can be no doubt therefore that the macaw is not so free from molestation in Costa Rica as I supposed it to be in Nicaragua. Whilst the statement respecting its immunity from attack will need modification, the argument I founded upon it may still hold good. Birds on the wing could not evade the keen sight of a hawk by any protective colouring, and if when at rest the macaw did not need concealment, natural selection would not work to tone down the colours that sexual selection tended to make more pronounced.

It will gratify all naturalists to learn that some of the results of Prof. Gabb's long and critical study of the miocene molluscan fauna of Santo Domingo and Costa Rica and its relation to the existing species of the Atlantic and Pacific Oceans, will shortly be ready for publication. Much light will be thrown by them on the interesting question of the time of the latest connection of the two oceans through the strait that once separated the northern from the southern continent.

THOMAS BELT

The Cedars, Ealing

"On the Question of Free-Will"

I SHOULD like to call the attention of your readers to what appears an important matter in connection with the above subject, which has attracted considerable attention of late, and which has also its physical bearings. In a recent lecture by Prof. Tyndall, the aspect of compensation and punishment for actions was treated of in connection with the question of free-will, and I think that it cannot but have struck many that the conclusions arrived at as regards this special point were less satisfactory or complete than the otherwise able reasoning of the lecture. What I should like to submit is that this special point is entirely independent of any question of free-will.

The argument is that if the will be *not* free, then reward for a good action, or punishment for a bad action cannot be *deserved*; but are merely *expedient*. I submit that the contrary holds true, quite independently whether the will be free or not. For it seems to me that the great point (that has apparently not been taken into account) is that the expectancy of the reward enters in as an element to *determine the will*. If there were no reward in prospect, the action would not be done. It must therefore be an error to argue that because the will is not free, therefore the reward is not deserved. To withhold the reward would be to reverse the conditions under which the action was willed.

In the same way as regards punishment. A person (say) for his own benefit appropriates to himself a sum of money. The person in appropriating the money contemplates the possible punishment, or takes this eventuality into consideration as an element in determining his will. If, therefore, the punishment were withheld, it would (quite independent of the question of free-will) be an injustice, because the person would derive a benefit without any compensating disadvantage. So in the same way in the previous case of the reward, he would (if the reward were withheld) undergo inconvenience without any compensating advantage. Thus I submit that rewards for good actions and punishments for bad actions have nothing to do with the question of free-will, for these in any case enter as elements in *determining the will*. Therefore punishment for an offence (like reward for a good action) is not merely an *expedient* thing, but in accordance with reason and justice.

Is not the question of free-will in itself rather a quibble? A man's will is dependent on his reason, or *will* may be said to be a special act of reason. Reason, it will be generally admitted, depends on brain structure. Else what are our brains for? Hence *will* depends on brain structure. Can it be said that on that account *will* is not free? For a man to be dominated (if conceivable) by a will independent of his brain structure, he would surely be a slave; for surely brain structure enters into the determination of a man's identity. So long as *will* is subject to brain structure, it is subject to reason, for brain structure is the mechanism of reason (or, at least, a mechanism *necessary* to reason). To have a will not subject to brain structure would be, therefore, to have a will not subject to reason (or a will that runs wild). Can any greater slavery be imagined than to be dominated by an independent will not subject to reason? I say, therefore, that *because* the will is subject to brain structure, *therefore it is free*; this, therefore, in direct opposition to the

opposite party who hold that, for the will to be free, it must not be subject to anything, *i.e.* must run wild independently of the controlling mechanism of brain structure.

The most powerful argument *against* anything is perhaps the argument of an exceedingly competent reasoner *in favour* of a wrong cause. Thus the portion of Sir John Herschel's lecture on "The Origin of Force," in which he supports independent free-will (so termed), constitutes the most powerful argument against it; as, in order to support his conclusions, he is obliged to assume the *creation* of (a small amount of) energy; or, to support independent free-will, he has to touch upon the perfection of the principle of the conservation of energy. It is a known fact that a man, however able, may not be an equally competent reasoner on *all* points. It may be observed that those persons who would maintain an independent free-will would thereby entirely ignore the beautiful mechanism of the brain, and suppose it useless. *Will* subject to brain structure (*i.e.* to the mechanism of reason) is surely free, for the emancipation of the will from reason would be anarchy or slavery. If, therefore, we admit that under no conceivable circumstances would we have the will otherwise than subject to reason, then even if we could conceive the will emancipated from brain structure, the will (if consistent with reason) would still be *the same* as when subject to brain structure; for brain structure, being the mechanism of reason, determines the *will*, and makes it consistent with reason. Therefore I contend that the question of free-will is a quibble, or the will subject to and determined by brain structure (the mechanism of reason) is perfectly free.

The subject is a difficult one, and I may, no doubt, have said some things that admit of improvement, but I should be glad to have in any way contributed to throw a true light on this interesting question.

P. Q.

London, October 16

Early Observations of the Solar Corona

THE "Astronomical Column" in NATURE, vol. xvi. p. 255, has drawn attention to an observation of the solar corona by Clavius during the total eclipse of 1605. This is, however, by no means the earliest known case in which the corona was remarked. Plutarch already had alluded to the faint light round the eclipsed sun, but the first eclipse, during which the corona appears to have made a strong impression on the observer, seems to have been that of August 31, 1030. On this day a fierce battle took place at Sticklestad, in Norway, between the Christian king Olaf (afterwards the national saint) and his heathen subjects. During the battle the sun was totally eclipsed, and a reddish light appeared round it. Before the eclipse of 1842 had made astronomers familiar with the corona and protuberances, Hansteen had suggested that it might be the zodiacal light which caused the red light in 1030.

Observatory, Birr Castle, Ireland

J. L. E. DREYER

Sense Perception of Electricity

IN the very interesting address of Prof. C. von Nägeli at Munich, on "The Limits of Natural Knowledge," of which a first portion is printed by you (NATURE, vol. xvi. p. 531), in illustration of his argument that there may be many forces in nature which we have not the requisite senses to perceive, he instances electricity as an universal element which might well have escaped our cognisance but for its occasional concentrations and disturbances making vivid appeal to two senses that we have—in lightning and thunder. The illustration is an apt and telling one, but is it worth while to note that though we have no sense differentiated to perceive electricity as the eye receives the light-wave and the ear the sound-wave of the circumambient ether (an organ, by the way, which would be useless to us unless we had also the power of self-insulation on the approach of this danger), we *have* a very general physical perception of electrical changes? The remark, for instance, is very common, "I thought it felt like thunder;" and in some this consciousness is quite abnormal. I knew personally one gentleman to whom this sensitiveness was such a constant source of *malaise* that he was medically advised to wear a fine silk vest as an insulator. In his case the success of the experiment was so marked that, according to his own statement, it "made life another thing." It would be interesting to know whether such a peculiarity was transmitted.

HENRY CECIL

Bregner, Bournemouth, October 22